

Panel discussion II: Reconciling observations and modeling of star formation at high redshifts

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Abstract.

This is the written account of the second of two panel discussions, on *Reconciling observations and modeling of star formation at high redshifts*. The chair of the panel was Pavel Kroupa, and panel members were Marc Balcells, John Beckman, Christopher Conselice, and Joseph Silk. After a short introduction by each of the panelists, panel and audience entered into a lively discussion, centered around the following six themes: the mass function of pre-stellar gas clouds; a possible top-heavy initial mass function at high redshifts versus mini-quasars as the first sources of ionization; the integrated galactic initial mass function; possible differences in specific star formation rates in disks and in massive galaxies; whether merging rates yield a wrong prediction for massive galaxies, and what is the physics behind the onset of the red sequence of galaxies; and the case of dark matter-dominated dwarf galaxies versus tidal dwarf galaxies.

KROUPA: Welcome to this second panel discussion, focused on observations and modeling of star formation (SF) in the high redshift Universe. Each of our panelists will start by giving a brief introduction to an aspect of this general topic that they find particularly interesting, and which they propose for further discussion. Let me ask John Beckman to start.

BECKMAN: The point I would like to make is not (at any rate not yet) on SF at high redshifts but on the initial mass function (IMF). The question is whether there is a universal IMF wherever and whenever stars have formed. It would imply, for example, that metallicities in stellar populations could be derived more precisely using that IMF. We have already heard during this week that nobody has found overwhelming evidence for variations in the IMF. But as absence of evidence is not evidence of absence can we conclude that the IMF is in fact universal? The reasons for suspecting that it might not be are that physical considerations seem to tell us that the IMF ought to depend on metallicity, gravity and interstellar medium (ISM) pressure, as examples. What I am about to say is detailed in a poster by Casuso (these proceedings, p. 000). In that work we were, using very simple assumptions, able to make an initial uniform spectrum of cloud core masses which is brought about by considering motion within a closed box, e.g., a spiral arm, and letting the system evolve. Using an algorithm in which the tendency of clouds in collision to merge or to fission is determined by their masses and their mutual velocities, we find results which agree well with what is measured locally in the ISM, but are also stable over time. Our model uses an arbitrary initial mass distribution of the clouds, and the output results depend only slightly on the input parameters, and very little on epoch after an initial relaxation time of order a few Myr. This model

is metallicity independent and virtually independent of dynamics. Its simplicity and robustness may enable us to gain some insight into the possible existence of a universal IMF.

BALCELLS: The confirmation that the color distribution of galaxies is bimodal has far-reaching consequences for our understanding of galaxy evolution. It has provided a simple, but inspiring, unifying theme for galaxy evolution. Galaxies are born and grow in the *blue cloud*, a sort of nursery for galaxies. Eventually, galaxies come of age, and migrate to the *red sequence*, a sort of retirement paradise, where massive galaxies live forever, growing only moderately by aggregation.

Irrespective of how much of this picture will eventually be shown to correspond with reality, one obvious question arises: what physical processes drive galaxies away from the blue cloud into the red sequence?

One basic hypothesis is that SF stops as a result of gas exhaustion, due to the ongoing SF. A second hypothesis is that gas may get consumed, removed and/or heated to virial temperatures during mergers. A third hypothesis, developed by Dekel and collaborators, exploits the presence of the most massive galaxies in the red sequence: the gas accreting onto dark matter halos along the filaments of the cosmic web remains cool when falling onto low mass halos, while it shock-heats to virial temperatures in galaxies with masses above about $\log(M/M_\odot) > 11.5$. This model amounts to a phase transition in the external interactions of galaxies. A final hypothesis would be that, as the potential of the galaxy deepens as a result of SF, eventually a phase transition occurs to the galaxy's ISM, and a new equilibrium, or quasi-equilibrium, is found at higher temperatures.

What makes the red sequence interesting is that it presents us with a lot of physics to analyze, in a territory where theorists and observers can contribute, compete, and, hopefully, lead us to a better understanding of the workings of galaxies.

CONSELICE: Figure 1 shows the mass and number densities for massive galaxies as a function of redshift. As can be seen, the densities for the most massive galaxies with $\log M > 11.5$ are nearly all in place, statistically, at $z < 2$. The two solid lines show the Millennium simulation results for the same quantities. As can be seen, the Millennium results underpredict the number of massive galaxies by up to two orders of magnitude. Although there is good agreement between the data and the models at $z = 0$, there is a large disagreement at higher redshifts, which continues to grow. This shows that the processes responsible for the assembly of massive galaxies in the Millennium simulation, which are generally mergers, occur much later in the simulation than for real galaxies. The formation of massive galaxies is therefore occurring much earlier than predicted. It is also observed that galaxy mergers are common at $z > 2$, and it is likely that galaxy merging is not occurring early enough in the simulations to produce the number of distant massive galaxies.

SILK: I wish to address the limitations of numerical simulations of galaxy formation and evolution. The problem is one of inadequate resolution. If one wishes to address SF one must go to very small scales and an enormous dynamical range - beyond reach currently. New rules are needed for sub-grid physics.

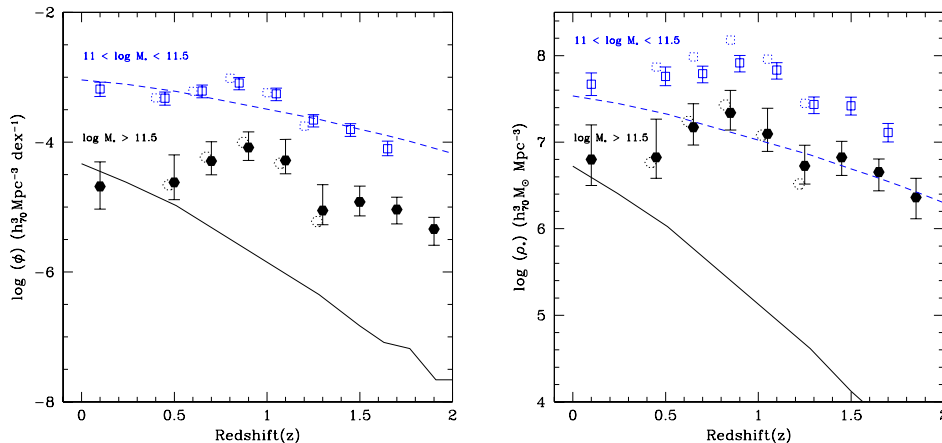


Figure 1. A comparison between Palomar NIR survey data (Conselice et al. 2007), and models from the Millennium simulation (e.g., De Lucia et al. 2006). The dashed line shows the predicted evolution in the number and stellar mass densities for $10^{11} M_\odot < M^* < 10^{11.5} M_\odot$ systems, while the solid line shows the same predicts for galaxies with stellar masses $M^* > 10^{11.5} M_\odot$. As can be seen, for the most part the simulated massive galaxies do not assemble quickly enough to match the observations.

Our current knowledge is based on observational phenomenology. However, detailed observations made locally are too complicated to implement, so assumptions must be made. These are then fine-tuned as more detailed knowledge becomes available, but it is a fundamental question as to how robust the predictions are. This is a general statement, but here are a few examples:

- There are three main problems with cold dark matter theory (CDM). These are the frequency of dwarf galaxies, of massive galaxies, and the ubiquity of halo cusps. The solutions to these problems may be tracers of early SF. Why is it that the cusps in galaxy centers cannot be erased by, e.g., a bar? Models look at the effect of the bar on its surroundings, but a bar is accompanied by inflow, and by continuous re-formation. So one must study the frequencies of dwarf and massive galaxies in different environments and metallicities, as well as the cores of halos. Any correlations obtained may lead to potential insights on fossil SF.
- Disks formed stars inefficiently but massive galaxies formed stars very efficiently. These insights are due to the derivation of specific SF rates (SFRs) from spectrophotometric modeling, and from $[\alpha/\text{Fe}]$ abundance ratio studies. One resolution may lie in invoking two modes of SF, with the inefficient disk mode being associated with supernova feedback and the efficient massive spheroid mode being at least partly regulated by AGN outflows.
- The first ionizing sources at high redshift may have been massive stars, possibly with a top-heavy IMF, or mini-quasars. Are intermediate mass

black holes required to account for re-ionization if the IMF is not top-heavy, and if so, what are their signatures?

My bottom line is that one should not believe current models, especially as far as their predictive power is concerned.

KROUPA: I'd like to bring up two additional problems which may well be associated with a currently wrong view of structure formation and the build-up of stellar content of galaxies:

1. Joe (Silk) already mentioned that CDM theory predicts the wrong number of dark matter dominated satellites of L^* galaxies. In this context I'd like to emphasize this problem further: we know that when late-type galaxies interact, tidal arms take away the angular momentum and energy. When these are gas rich, galaxy-sized objects are often observed to form in the arms; sometimes a dozen star-forming dIrr-type knots are evident per interacting galaxy pair. Theoretical work shows the number of self-gravitating knots to scale with the gas fraction. Okazaki & Taniguchi (2000) apply a standard hierarchical structure formation prescription for the build-up of galactic mass, and show that if only 1–2 long-lived tidal-dwarf galaxies (TDGs) survive per encounter involving late-type galaxies then *all* dE galaxies can be accounted for. In particular, their work shows that the observed morphology-density relations for both dwarf and giant galaxies in the field, groups of galaxies, and clusters of galaxies are readily explained. Okazaki & Taniguchi made very conservative assumptions on the number of TDGs formed, and the actual number of TDGs is likely to be much higher because the early galaxies had a larger fraction of their mass in gas.

My own work (Kroupa 1997; Metz & Kroupa 2007) shows that the interpretation that the Milky Way dSph satellites are dark matter dominated is probably wrong and that they are most probably ancient TDGs. Their spatial distribution about the Milky Way supports this conclusion (Metz, Kroupa, & Jerjen 2007).

Taken together, this would suggest a major logical problem with CDM theory because there would not be any room in the observed cosmos for any type of dark matter dominated satellite galaxy; CDM theory would predict the formation of more than enough TDGs to explain all known dwarf galaxies. The problem is that these TDGs cannot be explained away – they are an inherent consequence of fundamental conservation laws.

2. In cosmological applications the stellar IMF is often taken to be of an invariant Salpeter form. Sometimes top-heavy forms are considered. Recently it has emerged, though, that the stellar IMF of a whole galaxy, the “integrated galaxial IMF” (IGIMF) is a result of adding up all the stellar IMFs in all the star clusters that are forming. The IGIMF turns out to be steeper than the invariant stellar IMF and to depend on the SFR of a galaxy. Low-mass galaxies have steeper IGIMFs than massive galaxies and also a larger variation of the IGIMF. This has important implications for extragalactic astrophysics, as, e.g., the mass-metallicity relation of galaxies would be a natural result of such a variation of the IGIMF (Köppen,

Weidner & Kroupa 2007). Also, the $H\alpha$ -SFR calibration would be wrong for low-mass galaxies; the observed $H\alpha$ fluxes would imply significantly larger true SFRs than currently thought (Pflamm-Altenburg, Weidner & Kroupa 2007). Clearly, this needs to be taken into account in future stellar population studies.

I would now like to propose the following six points to discuss with the audience:

1. What is the mass function of pre-stellar gas clouds,
2. Can we distinguish between a top-heavy IMF at high redshifts, and mini-quasars as the first ionizing sources,
3. Is there a galaxy-wide IGIMF, along the lines of the description given by Weidner & Kroupa (2005) – we know the Salpeter description does not work,
4. Are there differences in the specific SFRs and in the $[\alpha/\text{Fe}]$ relative abundances between disks and massive galaxies,
5. Do merging rates yield a wrong prediction for massive galaxies, and how does this relate to the red sequence,
6. Are apparently dark matter-dominated dwarf galaxies really TDGs, or is theory trying to explain something that isn't there?

Can I invite comments from the audience please?

PAGEL: According to Kashlinsky et al. (2007), a small contribution to the extragalactic background light could come from supermassive population III objects for which they have measured an angular correlation function resembling that of galaxies.

KROUPA: And we should not just consider the dark matter paradigm, but also different gravitational laws to study where our understanding breaks down.

ALVES: If the characteristic mass function of the IMF is the critical Bonnor-Ebert mass (cf. Alves, Lombardi & Lada 2007), then I would naively expect, at least, a bottom-deficient IMF for the first stars, as $M_{\text{BE}} \propto c^2/p^{0.5}$.

SILK: The back-of-the-envelope calculation, which uses a sound speed approximation to H_2 cooling, indeed gives about $1000 M_\odot$ for the mass of the first stars to form in a metal-free environment. High dynamic range simulations of the first stars initially confirmed the naive predictions for the *minimum* mass. More recent simulations, which are now able to study more than one realization of collapse over a wide dynamic range, find a dispersion in the central temperature and density gradients that controls the accretion and angular momentum transfer rates. A range in first star mass is found, down to as low as $10 M_\odot$, and up to $100 M_\odot$. These stars are in the normal stellar mass range.

KRUMHOLZ: Theoretical models and simulations seem to suggest that the Arches cluster should have a top-heavy IMF, but observations have shown that

the Arches IMF is basically the same as everywhere else in the Galaxy. Given the failure of our models for the Arches, we should be wary of applying theoretical models that predict varying IMFs to the high redshift universe.

DOMÍNGUEZ-TENREIRO: I would like to comment on the issue of the top-heavy IMF versus mini-quasars as the first ionizing source. Cosmic flows can be described on a mathematical level by the Burgers equation, a generic equation in the framework of non-linear physics. This equation predicts that flows unavoidably develop singularities (i.e., black holes in the current context) under generic conditions. So, it is very likely that mini-quasars appear very early in the evolution of the Universe, and they would act as sources of the first ionization. Of course, this does not exclude a top-heavy IMF at high redshift, too.

PELETIER: I would like to reiterate an up to now unsolved problem: the low CaII IR triplet values in giant ellipticals. This was published by Saglia et al. (2002), Cenarro et al. (2003) and Falcón-Barroso et al. (2003). Assuming a normal Salpeter or Kroupa IMF, an average old age, and a high metallicity, necessary to fit absorption lines in the optical, the derived Ca abundance is lower than solar, unless the IMF is top-heavy at the low-metallicity end. Below solar [Ca/Fe] ratios are in principle unattractive, since Ca is an alpha-element, and alpha-elements are generally over-abundant with respect to Fe in giant ellipticals. The observations are beyond question, and the community is still waiting for a satisfactory solution for a line which is relatively well-understood.

SHLOSMA: I would like to respond to Joe's (Silk) apparent criticism of numerical simulations of dark matter cusp destruction by stellar bars. I fully agree that results of simulations should always be taken with a healthy dose of skepticism, *unless* they are supported by compelling theoretical arguments and common sense. In the particular case of the dark matter cusps, all numerical simulations with live bars (including our still unpublished results of $N \sim 10^{10}$ particles by Dubinski, Shlosman & Berentzen) show that the cusps do *not* dissolve (except in simulations by Holley-Bockelmann). Moreover, there are no compelling theoretical arguments as to why this should occur. The models now encompass pure collisionless cases as well as gaseous disks.

SILK: Proto-galactic bars in a gas-rich environment have not yet been adequately studied. I do not think the dust has settled yet on the issue of cusp softening.

VAZDEKIS: $[\alpha/\text{Fe}]$ overabundance ratios are usually assumed to be a result of highly efficient SF. But if the prediction by, e.g., Schneider et al. (2003) of a top-heavy IMF for metallicities lower than 10^{-5} is true, these overabundance ratios can be explained in part without invoking such high SF efficiency. Furthermore, a combination of the two scenarios might be appropriate for explaining the observed overabundance ratios.

KROUPA: But if one would have a top-heavy IMF, could one make a higher $[\alpha/\text{Fe}]$ without the need for a higher SF efficiency?

SILK: As you must also produce the Fe, you need type Ia supernovae, and so a top-heavy IMF will not be enough.

PIPINO: I would like to make two comments on points that were previously raised. Firstly, on Vazdekis' quest for population III stars, we showed (Matteucci

& Pipino 2005) that if you take population III stars into account as a real first (and single) generation of stars, you wouldn't notice their effect after $10^{11} M_{\odot}$ of stars have been created with a standard Salpeter IMF, over a period of at least half a Gyr. Secondly, on the Ca depletion in ellipticals: it is a false problem because Ca is also produced by type I supernovae in a non-negligible way (up to half of the total mass can be synthesized by supernovae Ia for a standard IMF), as is Si, therefore a ratio of $[Ca/Mg] \leq 0$ is naturally predicted by chemical evolution models (as described in Pipino & Matteucci 2004).

PELETIER: This paper still claims that Ca, as an alpha-element, is overabundant with respect to Fe, while in these ellipticals $[Ca/Fe]$ lower than solar seems to be needed. I agree that we have to look for the solution also in the direction of the chemical enrichment models.

HENSLER: As one learns from changes in element production in massive stars by refinements of the stellar models over the last years, a word of caution should be emphasized not to rely too much on abundances and their relations and take them too seriously as tracers, e.g., of changes in the IMF and SF episodes. So I agree that we should be more careful with interpretations of models until we have made sure that all the relevant physical processes are properly included, and until their influences on galaxies within the complex network of matter cycles are intensively explored.

HAMMER: I would just like to question the assumption that considering their specific SFRs, disk SF is not efficient. This is certainly true for local disks but what about the period six to eight Gyr ago? Some 15% of the M^* galaxy population is made with luminous infrared galaxies (LIRGs) which can double their mass in a few times 10^8 yr. And almost half of them look to be disks, some even with a normal-looking rotation curve.

BALCELLS: Maybe a way of addressing the question is to study whether LIRGs can be understood in terms of quiescent or burst-mode SF, or whether there is some continuum between the extremes.

HAMMER: Well, spectroscopy shows that in general LIRGs are the same as, or very similar to, spirals, which is a purely observational result.

KRUMHOLZ: I object, as I did in the first panel discussion and on other occasions, to the use of the term efficiency. Unless we have ironclad evidence that high-redshift SF does *not* follow the same rules as local SF, e.g., the universal IMF and the Schmidt-Kennicutt and Gao-Solomon correlations, we should not hypothesize the existence of additional "modes" of SF. It is significant that local LIRGs and ULIRGs follow the same correlations as normal galaxies, and, from what we can tell, high-redshift ones do too.

HAMMER: How do you know this? There may be no different physics at high z but certainly different conditions (gas, merging occurrence, etc...).

KRUMHOLZ: I repeat, the basic point is that we should only introduce new physics if we really have to.

SILK: So ULIRGs lie on the Kennicutt law?

KRUMHOLZ: Yes indeed, local ULIRGs do lie on both the Kennicutt law and the Gao-Solomon law.

BALCELLS: From the point of view of a galaxy observer, there are indeed different modes, for instance, ellipticals have an $[\alpha/\text{Fe}]$ ratio that is not found in disks, which implies the existence of a burst versus a quiescent mode. So I think we can talk about two modes. The two modes must reflect different physical conditions – though not necessarily new physical processes.

STRINGER: Just a short comment: do different modes of SF necessarily need to correspond to different IMFs?

BECKMAN: Not necessarily.

SILK: Could be the IMF, or it could be due to AGN.

KROUPA: This is a question of interpretation – some panel members would suggest no. Personally I think that under extreme star-forming rates ($> 10^2 M_\odot \text{yr}^{-1}$) a top-heavy IMF does emerge. One indication of this is that the metallicity distribution of the Milky Way bulge and of the M31 bulge are easily reproduced with a top-heavy IMF, while a universal IMF does not match the observations (Ballero, Kroupa, & Matteucci 2007). In Bonn we are working on constraining the IMF for extreme starbursts. It is too early to state definite results, but a top-heavy IMF appears to be necessary.

KROUPA: I need to start wrapping up, and was wondering whether anyone wants to comment on our fifth point, on merging rates and the red sequence.

CONSELICE: The merger rate or fraction is a very difficult parameter to determine, and observations presently disagree with theory. Much more work needs to be done before we can make statements on this.

TRUJILLO: The small sizes of galaxies at a redshift of around 1.5 are not in contradiction with current N -body and semi-analytical simulations. A couple of major mergers will locate these objects within the local stellar mass-size relation (Boylan-Kolchin, Ma & Quataert 2006; Khochfar & Silk 2006).

CONSELICE: The smaller sizes of these galaxies also demonstrate that merging is occurring, but how the observed merger rate, itself, agrees or not with the models still needs to be determined as there is no clear cut answer at the moment.

KROUPA: Let's hear a final comment from the audience before we turn back to the panel?

HAMMER: There is a lot of emphasis on the reproduction of the merger rate by simulations, but there is also a question to observers. In 1999 we made the first observation of pairs and of the merger rate up to a redshift of unity in the framework of the Canada-France Redshift Survey (CFRS). Since that time a very large number of papers has appeared on the subject, with different assumptions and methods (evolution-corrected or not, using blue light, red light, etc.). The overall result, also on the observers' side, is real confusion. I suggest that we should resolve this confusion before going to different simulations which also have various predictions. Otherwise, this "problem" will never end!

BALCELLS: The observational results can also usually be changed with sample selection. In work from our group, B -band vs K -band selection yields

different z -evolution of the merger fraction, even when I -band asymmetries are used for both diagnostics. Pixellation and cosmological dimming need to be carefully calibrated when using asymmetries or pair fractions to infer the z -evolution of the merger fractions. Going beyond $z \sim 1$ becomes especially tricky as CCD-based imaging surveys start to sample the rest-frame UV continuum. Given these difficulties, disagreement between different observers is almost unavoidable.

CONSELICE: Merger fractions can be calculated in various ways and need to be determined quantitatively before comparing them with observations.

SILK: On the one hand the merger rate does not agree well with the models. But on the other, I am sure that modelers are very clever and so will come up with new models which will fit much better. But the question is whether we will learn any new physics from it?

KROUPA: We have run out of time, so before finalizing this panel discussion I would like to ask John (Beckman) to make a final statement.

BECKMAN: It is refreshing to see so much discord about the IMF. There are good reasons for uncertainty about the question of whether under extreme star-forming conditions or under conditions of very low metallicity the IMF is non-standard. These have been mentioned in the discussion and are as always a question of how to interpret the observations, notably of element abundance ratios. The question of whether population III occurred or not is closely linked with this and is also up for grabs for the same sort of reasons. It is also refreshing to find that there are no clear answers to the problem of the lack of cuspieness in galaxy centers, or indeed to the question of why the semi-analytical models, with mergers at their heart, give such a relatively poor account of the evidence from stellar populations of the way and the rates at which galaxies have evolved. All this bodes well for the next generation of researchers.

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